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the tree, that the young larvæ may avoid destruction, — the bird, meanwhile, is by a like careful selection, acquiring claws fitted to climb, and a beak fitted to pierce the bark, and so has become a woodpecker. After all the prolonged and patient efforts of Nature, through countless ages, the relative numbers remain precisely at the point from which they started.

Finally, if this theory is true, it should be carried much farther. For why stop at the limits of human vision? Why at those of the best microscopes? Why even at those which we may expect the microscope ultimately to attain? Beyond and below these, there may exist myriads of forms, myriads of created organisms, equally entitled, on all principles of reasoning, to claim that they have been formed in the image of that original pair.

Four hundred and eighty-third meeting.

May 8, 1860. — MONTHLY MEETING.

The PRESIDENT in the chair.

Dr. Kneeland, in reference to some criticisms which his communication at the last meeting, upon the barking of dogs, had called forth, remarked, —

That, as regards the testimony adduced, which he said was the same as had been extolled on the other side of the question, he had introduced the testimony of the same hunter-naturalist, and his only, to show that the wild dogs in question were widely different from the common type of dogs, and that their voice could not be fairly compared to the educated bark of domesticated dogs.

As to the occurrence of indigenous wild dogs south of the Equator, he maintained, on the authority of Hamilton Smith and others, that the South American wild dogs are aguara or fox-dogs, and not true dogs; and also, on the authority of many naturalists, that the South Pacific dogs have been introduced from the Asiatic continent by their Polynesian masters; that, according to Dr. Pickering, there is probably no aboriginal dog in New Zealand; that the dogs of the Namaqua region in Southern Africa, on the authority of Anderssen, are half-reclaimed jackals; and that the Australian dingo, an exception to the zoölogical character of that region, on the authority of Dr. Carpenter and others, is more likely to have been introduced from Asia, and to be the progeny of the Indian dhole, (rendered the more probable by the wolf-like characters of the dingo,) than to be the sole indigenous, carnivorous, placental mammal on that continental island. Seeing that it must be a mere matter of opinion, he considered the question of the occurrence of indigenous wild dogs south of the Equator as at best still *sub judice.*

Mr. C. Wright made some remarks on the architecture of bees, in reference to previous discussions upon the instinct of the honey-bee.

Mathematicians have regarded the economical characteristics of the honey-cell too exclusively, to the neglect of those symmetries which Maraldi pointed out.

The more prominent of these symmetries are the regularity of all the solid angles of the cell, and the consequent equality of all the angles made by the sides and rhombs with each other to 120°, or to $\frac{4}{3}$ of a right angle. Another important symmetry which follows from these is seen in the position of that point in the axis of the cell which is directly over the middle points of the rhombs; for this point is at the same distance from all the nine planes of the cell, and just opposite similar points in the nine contiguous cells; so that little spheres which would just fit the honey-cells would, if pressed to the bases of the cells on both sides of the comb, touch the rhombs in their middle points, and the sides in their middle lines, by points in the spheres themselves, at which they would touch each other but for the thickness of the intervening walls.

While the common mode of considering the form of the honey-cell regards it as the effect of rational economy, these symmetries show how the cell might be the natural result of simple or sensible economy, as applied to the building of simple nests, the common type of which is a cylindrical cavity with a hemispherical base. The construction of a series of such nests side by side, and with the bases of two opposite series in closest contact, would, by the simple removal of the interstitial material, result in two series of cells like the normal ones of the honey-comb, both in the forms and the arrangement of the sides and bases. Hence, as the bee builds the two series of cells from their common bases, making the incipient depressions on one side form the interstitial elevations around the cavities of the other side, and as it builds by continual trimming and saving, we may infer that the form of the honey-cell does not require, in the bee's instinct, any reference to supersensible properties of form, but only a reference to sensible economy and facility of construction; especially as no one would contend that the utility, to innumerable nest-building animals, of spherical and cylindrical surfaces, depends upon their economy (which is still greater than that of the honey-cell), rather than upon their far more obvious symmetries and facilities for construction. It appears, therefore, that the instinct of the bee does not differ in kind from instincts in general.

Mr. Newcomb discussed the objections raised by Mr. Mill, and others, against Laplace's presentation of the doctrine of probabilities.

Many objections have within late years been brought against the fundamental basis of the theory of probabilities as laid down by Laplace. Some of these proceed from an entire misapprehension of the mathematical and logical signification to be attached to the term *probability;* others from a defect in Laplace's theory considered as a philosophical structure, which, although not necessarily leading to any error in the treatment of any special problem, has nevertheless been adduced as a reason why Laplace fell into the assumed mistakes. However, as it is conducive to sound reasoning to have the fundamental principles of every deductive science laid down with as much logical clearness and accuracy as possible, let us first consider the question, What is probability?

The probability of a proposition has sometimes been defined as the *amount* of our belief in the truth of that proposition. Here, however, a difficulty arises from the fact, that neither belief, nor any other affection of the mind, admits of being measured as a *quantity.* We may apply the terms greater or less to belief, but we cannot say *how much* greater or less. Nor does this inability proceed from the imperfection of the faculties, as when we experience a difficulty in determining by the feeling whether one weight is twice as heavy as another. It is inherent in the nature of things. In order that we may say of one thing that it is twice as large as another, we must be able to conceive of it as susceptible of division into two independent parts, or as formed by the superposition of two such parts, each of which can be considered separately. Now it is obvious that belief cannot be considered as formed by the superposition of several less beliefs.

But although belief itself cannot be considered as a quantity, we may submit to mathematical computation those combinations of circumstances which induce partial belief. The whole mathematical doctrine of probabilities may be considered as founded on the following definition: —

If of m events *one* and *one only* must occur (or have occurred); and if an individual is entirely ignorant of any reason why one of these events should occur rather than another, and if n of these events belong to a class A, then we call the fraction $\frac{n}{m}$ the *probability*, for the mind of the individual in question, that the event which will occur, or has occurred, belongs to the class A. The probability might equally have been expressed by the fraction $\frac{n}{m-n}$; but this would be less convenient for mathematical computation, although *more* in accordance with the language of common life.

The solution of every possible mathematical problem in probabilities consists simply in determining, from the conditions of the question, the values of m and n, or rather of their ratio.

It will readily be perceived that probability, as thus defined, (and this is equivalent to the usual definition,) is not a quality inherent in the event itself. The latter may be determined by laws as exact as those which regulate the motions of the heavenly bodies. The principle that every event which occurs is the result of law, and neither has or ever had any *absolute* uncertainty inherent in it, may be regarded as an induction almost as perfect as the laws of motion.

Suppose that a die is loaded on one side. As long as we are entirely *ignorant* of any reason why the load should be on one side rather than another, the probability of any one side turning up will still be $\frac{1}{6}$ by definition. If we found the same side of a die to be thrown four times out of five, we should be disposed to inquire whether the frequency with which it was thrown was the result of *chance* or of *law*. Expressed in exact philosophical language, our question should be as follows: —

Were these successive sixes the result of separate and independent causes, conditions, or determining reasons?

Or was there a common element in the causes or determining reasons which produced them?

But, it may be asked, is not the shaking of the box by the same individual in either case a common element in the causes of all the throws?

I answer, that we are not to look for the common element in those causes which are concerned only in producing the whole class of effects, the relationship of which we seek; but in those *additional and unknown* circumstances which determine why *one* of these effects should be produced rather than *another*.

Now, the shaking of the box is supposed simply to cause the result,

A side is thrown,

while the cause of the side *six* being thrown may be a loading of the die, or an ability in the thrower to cause what side he pleases to turn up, either of which would be a common cause, acting through the several throws, and having a special relation to the side six.

The proportionate number of times which an event will occur in a great number of times is called by Laplace its *facility*, which word may be defined as follows: —

If a constant system of causes is combined within an infinite succession of *independent* causes; and if, of the infinite number of effects thus produced, a certain proportion belong to a class A, then is the *facility* of production of an effect of that class represented by the fraction representing the above-mentioned proportion. Or, we might say that the constant system of causes is combined with every possible independent cause, and represents the facility by the fraction expressing the ratio of the effects of the class A to the whole number of effects.

We easily see that the facility of an event depends on the constant causes alone. It is therefore the probability of the event to a mind acquainted with all the constant causes, or with the laws of their action.

Owing to the complicated manner in which the constant and the variable or independent causes are combined, and the general vagueness and difficulty of definition of the former, we can seldom determine with accuracy the facility of an event, except in a few simple mathematical cases. Thus, if we wish to construct a perfect table of mortality, if we select our lives from individuals of nearly the same place and time, the examples will not be sufficiently numerous to assure us that the independent causes have acted in every possible way; and if we select them from many countries, and during a long course of time, the circumstances of climate, general external condition, &c., which ought to be constant, will differ with the different individuals.

A few examples may serve to elucidate the above principles.

A bag contains black and white balls in an unknown ratio. The *probability* of drawing a white ball will be one half. But the *facility* will be represented by the ratio of the white balls to the whole number, supposing them to be mixed indiscriminately, and when this ratio is known, it will also be the probability.

So long as the deaths of individuals in any particular country all proceed from independent causes, we expect their number to agree with those deduced from a table of mortality, within certain limits of error. If, however, the country were ravaged by an epidemic, or any change were to take place in its climate, we should no longer expect the deaths to follow the laws of the tables of mortality. Here, however, would be a *single cause*, producing, or effecting in some way, the deaths of a number of individuals.

The neglect of the distinction between probability and facility has never led to any error, (except, perhaps, of interpretation,) because every answer to a question in chances must represent a *probability*. If we are ignorant of any of the constant causes, we can only deduce *the most probable value of the facility*, which may not be the real value; — if we know the law of action of all the constant causes, the probability and facility are the same.

Let us now consider a question which has been the subject of some dispute, viz. the nature of the argument by which, from the fact of the near approach of several stars, it is deduced that their proximity is not the result of chance. Writers on probabilities have generally agreed that such proximity indicates a physical connection between the stars.

The real argument for the connection is to be expressed in the following form: —

1. If the causes which fixed the position of each star were entirely independent of those which fixed the position of every other; or, in other words, if the stars were scattered by chance, it is *exceedingly improbable* that these (two, three, or seven) stars should be found as near each other as they actually are.

2. It is *not* very improbable that such proximity should result from the existence of a common element in the causes which determined their positions.

3. Therefore, such proximity being an ascertained fact, it is in the highest degree probable that it resulted from the existence of a common element, etc.

This reasoning is logically perfect. The first premise (if I may be allowed to call it so) is proved mathematically; the second is considered evident, and has seldom or never been expressed by writers on the subject, and the conclusion follows from the obvious principle, that, when compelled to adopt one of two possible suppositions, we select the most probable one.

The most elaborate argument in favor of the first premise is that given by Mr. Michell in the Philosophical Transactions for 1767. Professor J. D. Forbes of Edinburgh has taken some exceptions to Michell's methods, and to the general logical accuracy of his method of treatment, besides pointing out one of two mathematical errors in his computations,* in an article in the Philosophical Magazine for December, 1850. He raises the following objections: —

" FIRST OBJECTION. — *The doubt existing in the mind of a reasonable person whether an event still future, and which may happen many ways, shall occur in a particular given way, is erroneously considered as equivalent to an inherent improbability of its happening or having happened in that way.*"

SECOND OBJECTION. — *To assume that every star is as likely,* not hypothetically, but actually, *to be in one situation as another, leads to conclusions obviously at variance with the idea of random or lawless distribution, and is therefore not the expression of that idea.*

He also negatives the following conclusions, which he conceives must be maintained by Michell's followers: — 1. That there is any *calculable* probability, such as 9570 to 1, against the observed occurrence of two stars out of more than 1000 within 4″ of one another having been fortuitous. 2. That the fact of two stars being seen within an infinitely small distance of each other amounts to a mathematical proof of the *certainty* of their being physically connected. 3. That were the stars uniformly spaced over the heavens, or arranged with perfect symmetry, no argument could be alleged against such arrangement being the result of chance, but any deviation from symmetry would raise such an argument.

With regard to the first objection it may be remarked, that, except the word *inherent*, what Professor Forbes objects to is equivalent to the very *mathematical definition* of the word *probability*. It seems likely that he uses the word in that sentence to express the idea or entity which I have called *facility;* if so, the objection is well taken *against the general principle.* It does not, however, affect Michell's *result.*

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* Neither of these errors affects the *general character* of the result.

The second objection is also aimed at what seems to me not only a mathematical, but a common-sense definition. If I correctly understand it, Professor Forbes attempts to sustain it by showing that it follows from the assumption alluded to in his objection, that a *uniform distribution* of the stars is that which would be most probable as the result of random scattering. This is true, but the *most probable* result of a trial may be *almost infinitely improbable.* We shall consider this more fully hereafter.

In the first conclusion denied by Professor Forbes, it is not made clear whether is meant (1.) the *a priori* probability that such an event *would occur* as the result of chance; or (2.) the *a posteriori* probability that, having occurred, it was the result of chance; or between the first and third propositions in the method of reasoning cited above. Let p be the *a priori* probability of the first proposition, l the *a priori* probability that the resulting proposition would result from some law, or of the second proposition. Then by the fundamental theorem of the probabilities of causes, the probability that the observed contiguity is the result of law is $\frac{l}{p+l}$, and the probability that it is the result of chance is $\frac{p}{l+p}$. Now, as above remarked, it is tacitly assumed by nearly all writers, that l, though a small fraction, is *very great* compared with p. Now p is, *to a certain extent,* capable of being expressed in exact numbers, but l is not; therefore $\frac{p}{l+p}$ is not. Professor Forbes is therefore correct if he refers to the second of the above meanings, as was remarked by Professor Boole in a subsequent number of the Philosophical Journal. At the same time, however, we may, by the aid of numerical calculation, make an *approximate* estimate of the probability of the proposition used in its second meaning.

The second proposition is a *demonstrable mathematical* certainty, *unless* it be held that it is *infinitely improbable* that two stars should be *infinitely near* as the result of law, which I apprehend no one will maintain. In fact, p will become infinitely small, while l remains finite, so that $\frac{p}{l+p}$ will become infinitely small.

The third proposition does not follow at all from Michell's argument. A certain calculable amount of irregularity, or grouping, is to be expected as the result of a random distribution. If the amount of grouping is much greater than this, it indicates that the components of each group were together in consequence of some common cause determining them to nearly the same position. *Vice versa*, if the stars are equally spaced over the heavens, it would indicate that some *constant cause* had operated, tending to *prevent them from occupying positions near each other*. Law and chance are not necessarily the antithesis of each other in the mathematical expression of their effects. Law is indicated by a deviation from what ought to be the results of chance, in whatever direction this deviation may be.

Let us in this connection return to the second objection of Professor Forbes, to see in what sense a uniform distribution is the most probable result of chance. It is so only when, supposing the heavens to be divided into a given number of equal spaces, we are required to specify exactly how many stars each *special division* contains. Suppose the heavens to be divided into 100 portions, and 200 stars to be distributed at random; then, if a person is required to guess how many stars the first space contains, how many the second contains, and so on to the hundredth, he ought to guess two for each space. Yet the chances are *millions to one* against the correctness of such a guess; but they would be *still greater* were he to guess differently. But suppose he were simply required to guess *how many spaces* contained no stars, how many contained one, &c., *without specifying the particular spaces* which contained the several numbers. Theory shows that, in the case supposed, the probability that a space selected at random contains n stars is very nearly $\frac{2^n}{n!\,e^2}$, e being the Neperian base. The individual ought, therefore, to guess that 14 spaces were devoid of stars,

27 spaces contained 1 star each,
27 " " 2 "
18 " " 3 "
9 " " 4 "
4 " " 5 "
1 " " 6 stars.

This guess, however, would include a number of guesses of the first class, equal to the number of possible permutations of 100 things, 14 of which were of one class, 27 of another, &c., to express which number would require *ninety-seven significant figures*. Yet the guess would in all probability be wrong in some respects, although there is no reasonable probability that it would differ *much* from the truth.

As another illustration, suppose that a die, of which four sides are white, is thrown thirty times. If the die is fair, white every time is evidently more probable than any other system of throws, supposing that in specifying the system we state *what color is thrown on each specific trial.* Yet such a result would prove beyond all reasonable doubt that the die was not fair. This statement will not appear paradoxical, if we consider that a constant series of throws of one color would *not* be very improbable on the supposition that the die was loaded, while they would be very improbable on the supposition that the die was fair. We therefore select the most probable series of circumstances, and say that the die is not fair. If an indiscriminate series of white and black throws result, the former being about twice as numerous as the latter, such result, describing what color was thrown on each throw, would be still more improbable than a constant series of whites on the supposition that the die was fair. But the former result would be *billions of times* less probable than the latter on the supposition that the die was loaded; so that, if the former result occur, we select the supposition of a fair die as the more probable.

It will be perceived that the *degree of speciality* with which a phenomenon is described affects very materially its *a priori* probability, but the full development of the results of this fact is reserved for a paper on the applications of the theory of probabilities to natural phenomena. I may remark, however, that much confusion has arisen from confounding the different degrees of probability which a proposition will have when expressed in the different forms,

$$A \text{ is } X,$$
$$A \text{ is } V,$$
$$A \text{ is } Z, \text{ \&c.},$$

when all V is X, all Z is V, &c., but V is an exceeding small portion of X, Z of V, &c. In such a case the *a priori* probabilities of the successive propositions will diminish with great rapidity. If, in the case of the above-mentioned die, supposed fair, one were to guess that the 2d, 5th, 8th, &c. throws would be black, and all the rest white, he would be 1024 times more likely to be wrong than if he guessed that they would all be white. But if he guessed simply that twenty throws would be white, and ten black, which guess would include the former, he would be myriads of times more likely to be right than if he guessed that they would all be white.

Professor Peirce made a communication upon the grounds of the probability of intermercurial planets, and upon polar forces.

Professor Agassiz gave an account of his recent investigations of the structure of the Radiata, and discussed their general homologies in reference to the natural limits of that branch of the animal kingdom. He proceeded to a comparison of their special homologies, and concluded that the Radiata embrace only three distinct classes, the Polyps, the Acalephs, and the Echinoderms; that the Hydroids are genuine Acalephs, as well as the Ctenophoræ, and that the Siphunculoids must be excluded from the type of Radiata.

An oral communication was received from a delegation from a committee of citizens engaged in promoting the contemplated expedition of Dr. Hayes to the Arctic regions, and the influence and good offices of the individual members of the Academy were solicited in its favor.